DISCUSSION

ROBERT D. DUNN, M. D. (300 Hamilton Avenue, Palo Alto).—There is no doubt that the high fetal and maternal morbidity and mortality in this country are due in part to untimely interference by the obstetrician. This morbidity and mortality usually occur when interference is attempted with the head at too high a station and the cervix not fully dilated. If patience could be the watchword when the fetal heart shows no embarrassment to the child and the maternal pulse indicates no deleterious effect on the mother, labor would often advance surprisingly normally. This patience, of course, is difficult to practice even in moderately long labors, when one has the family of the patient constantly demanding that the doctor "do something."

In modern obstetrics this situation is most easily handled by the use of analgesia. The patients under influence of medication do not suffer, and thus permit more prolonged labor. This added time will decrease the incidence of mid-forceps considerably, but will increase the use of outlet forceps. The latter, however, in trained hands, cannot be considered a factor in increasing either maternal or fetal mortality.

High forceps should have no place in modern obstetrics. Although the mortality of cesarean section is high, that of high forceps is greater. If a patient is infected, with the head still not engaged, either a Latzko extraperitoneal cesarean section or a uterine marsupialization is safer than a high forceps procedure.

Conservatism does not imply a strictly *laissez faire* policy. It is most essential that the actual condition of the case at hand be understood. Then, if necessary, skillful and timely intervention is just as much a part of conservative treatment as the nonoperative approach to normally advancing cases.

If the author's plea for conservative obstetrics is to be effective, it is important for us to teach our medical students that interference in an obstetrical case is as serious as undertaking a major surgical operation. Thus, it should be attempted only after careful consideration, and if there is any question as to the advisability of such a procedure, consultation is often a help and always a protection.

THE LURE OF MEDICAL HISTORY†

TRUTH OVERTAKES "DOCTOR HUNTER"

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PART I

"WHATEVER the truth may be, it is best that we should know it; and for truth of any kind we should keep our heads and hearts as cool as we can." Thus wisely counseled James Anthony Froude, in his admirable essay on the science of history. Since the two great Scotsmen, John and William Hunter, have been dead so long, it should not be difficult for anyone to keep his head and heart cool, with respect to the controversy between them. And, as for the truth in the matter, it has been available though apparently unrevealed ever since 1762. It is found in the apparently forgotten though crucial words of William himself. Although we had tried to learn the facts regarding the bitter controversy, this damaging footnote was not mentioned in the biographies, essays, addresses, Hunterian lectures and orations consulted. And, although Paget severely characterized the *Medical Commentaries* which contain it, and discussed the well-known estrangement, this footnote apparently had no special significance for him.

† A Twenty-Five Years Ago column, made up of excerpts from the official journal of the California Medical Association of twenty-five years ago, is printed in each issue of CALIFORNIA AND WESTERN MEDICINE. The column is one of the regular features of the Miscellany department, and its page number will be found on the front cover.

AUTHOR'S FORMER REFERENCES

In a footnote to Essays on the History of Embryology,[1] I incidentally expressed sympathy with John in his quarrel with William. I felt prompted to do so after carefully reading John's contribution, of 1780, to the Royal Society of London, entitled "On the Structure of the Placenta," and pondering the letters written by these gifted men regarding it. A few years later, when considering the work of John in embryology, I further stated:

It is difficult to contemplate the splendid royal folio on the gravid uterus[2] by William without sympathy for John. It does not seem possible that the latter could have said what he did, and taken the steps he did more than a generation afterward, if he himself had not made the discovery he claimed, regarding the uteroplacental circulation; and surely nothing could be more evasive than the rejoinder of William. John was very specific and said that William received his conclusion with raillery at the time.[3]

When discussing the contributions to embryology of the Hunters, I reverted to the subject, adding:

It may have been fortunate that John was probably unaware of the views of "eminent anatomists" referred to by Haller on this matter, unless he could also have known what Aranzi and others had thought, and what Falconnet and others [especially Monro, Sr.] had done, in order to solve the vexed problem of the uteroplacental circulation. Concerning the controversy between the two, Teacher regarded the account of William more probable than that of John. [However], one cannot contemplate the rejoinder of William to John, made to the Royal Society on February 3, 1780, without noting its evasion and ambiguity. It must be wholly unconvincing except to those to whom the possession of stolen goods is conclusive proof of their rightful ownership, for that is a form of reasoning resorted to by William. Moreover, one cannot help wondering what experience William had that caused him to declare to his students that ". . . most philosophers, most great men, most anatomists, and most other men of eminence lie like the devil." . . . it is well to recall William's words regarding the fetal and maternal portions of the decidua as quoted by Teacher (p. lvi), which are to the effect that the vessels of these two parts are separate because "those of the umbilical always 'remained uninjected.' 'It was this appearance,' he says (in his lectures of 1775), 'in the cat and bitch that first led me into the apprehension that the human placenta was the same. I thought this for a long time, but I never cared to assert it openly till within these few years.' "[4]

COMMENT

Since John claimed that he discovered the independence of the uteroplacental circulations in 1754, the last sentence in the above quotation alone would seem sufficient to dispose of William's claim. But, had he really thought so "a long time," that is, before 1754?

This [1755] was within a year of the time when he [John] had the exceptional opportunity to study and dissect a human uterus with fetus near term, injected by McKenzie, and the occasion on which John apparently rediscovered the independence of the maternal and fetal circulations. It seems that the cadaver had been injected through the uterine, and the unborn child through the umbilical vessels, and John said that he conceived the idea of the independence of the circulations while dissecting the placenta. Although he later wrote that his elder brother received the idea with "raillery," when

1 California and West. Med., Vol. 36, No. 6, p. 394 (June), 1932.

2 Hunter, William: Anatomia uteri gravidæ. Birmingham, 1774.

3 California and West. Med., Vol. 43, No. 5, p. 362, col. 1, par. 2 (Nov.), 1935.

4 California and West. Med., Vol. 46, No. 1, p. 38, par. 2 (Jan.), 1937.

he broached it to him at the time, it is a well-known fact that William maintained that it was his own discovery, and that he always had incorporated the idea in the first and only public lecture in the fall of every year. [But] had William sponsored the idea of the independence of the two circulations before 1754, it is extremely unlikely that John would have remained unaware of it, and he, hence, would have had no occasion to think that he had made a discovery when dissecting the female body injected by McKenzie, and so report to William. Moreover, according to Paget, the manuscript notes taken by one who attended William's lectures in 1755–1756 show "that William Hunter, even then, a year after the discovery of the placental structures, neither spoke of it nor understood the meaning of it"—the uteroplacental circulation. These words also are in complete accord with those quoted above from Teacher, and also [with] those from the anonymous auditor apparently written after the death of William, and, if true, fully confirm John's statement regarding William's reaction to the idea. Moreover, careful scrutiny of the text written by William himself for his famous folio, published in 1774, twenty years after John's rediscovery, confirms Paget's view completely, for the independence of the two circulations is only implied, not represented or asserted there.[5]

When considering the controversy further, I wrote:

". . . it may be recalled that William said that his 'discovery' of the independence of the fetal and maternal circulations had been acknowledged by Haller thirteen or fourteen years before the disagreement with his brother John in 1780, in Haller's *Elementa physiologiæ corporis humani*,[6] Vol. 8, p. 220, which appeared in 1766. However, instead of having credited William with the discovery, Haller merely gave a brief summary of William's ideas in an addendum, retaining the contrary idea in the text and saying that Hunter's ideas regarding the decidua have been 'partly corrected and partly confirmed.' According to Haller's statement in this addendum, William believed that a liquid injected into the vessels of the uterus 'pours into every cellular part of the placenta, and from these cells returns into the broader veins of the uterus. None of it passes into the branches of the umbilical vessels. When the umbilical artery is filled with a colored liquid, while the placenta still adheres to the uterus, the branches of the umbilical arteries and veins are all completely and readily filled; yet nothing passes into the vessels of the uterus, unless the liquid has poured into the cells of the placenta.' Haller, hence, merely stated that William believed that the two circulations were independent without characterizing the idea as a discovery. Since this was written in connection with a historical summary of the idea, it is not without significance."

What is of special significance, however, is the fact that it was in this connection that Haller referred to page 52 of the *Medical Commentaries*[7] of 1762 where William stoutly maintained the opposite view.

OTHER COMMENT

Teacher (1900) concluded that ". . . it is unreasonable to suppose that they [the injections] were figured then [1750], yet only understood in 1754." (p. lvi.) But it seems equally strange that William should have remained silent in 1774 in regard to what he said, in 1780, he so firmly believed and thought he had discovered [before] 1754. I do not know upon what grounds Teacher (1899) stated that William's conclusion regarding the distinctness of the maternal and fetal circulations "was, as Hunter was aware, strictly speaking, not a new discovery" (p. 32), but if justified it robs William of his claim and reveals him in disparaging light. It may well be true, as Teacher held, that William Hunter presented the anatomical proof that the two circulations do not mingle, yet the real question is not whether he presented this proof, but whether he fully accepted it for man, before John, and that does not seem to have been the case. This point is referred to by an anonymous auditor of William's lectures, who recorded him as saying that

These appearances I first saw in a cat that was pregnant, which I injected from the uterine vessels, after which I injected by the navel string. The injection was distinct; I afterwards saw the same in a bitch. These discoveries were early in life, and taught me to believe that the same course of things existed in the human species, though I could not demonstrate it, not having had an opportunity to inject a pregnant uterus; but soon after I did meet an opportunity, & the appearances staggered me. For after injection of the parts, I found the uterine injection filled up a great part of the placenta &c., which on examination proved to be spongy & cellular. (p. 82.)

Hunter no doubt was "staggered," as well he might have been, because the injections of pregnant human uteri, instead of being unequivocal as in the cat and dog, were confused and seemed to controvert the conclusion reached from injections of the carnivora mentioned. However, according to Needham, "His [William's] injections left no shadow of doubt about the matter, and the way was clearly opened up for the study of the properties of the capillary endothelial membranes separating the bloods . . ." (p. 201.)

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(To be continued)

5 California and West. Med., Vol. 46, No. 1, p. 39, col. 2, par. 1 (Jan.), 1937.

6 Von Haller, Albrecht: Elementa physiologiæ corporis humani, Vol. 8, p. 220.

7 Hunter, William: Medical Commentaries. Part I. Containing a plain and direct answer to Professor Monro, Jr., interspersed with remarks on the structure, functions and diseases of several parts of the human body, VII, 113 pp. 4° London, A. Hamilton, 1762.

BUBONIC PLAGUE OUTBREAK IN SAN FRANCISCO—YEAR 1900*

THE first severe test of the competency of United States Public Health Service bacteriologists may be said to have occurred in 1900, when suspected cases of bubonic plague began to come to the attention of the local sanitary authorities in San Francisco. Dr. Joseph J. Kinyoun, formerly the director of the Hygienic Laboratory, was then assigned to quarantine duty at that port. It was his duty to be informed of local health conditions, and he obtained materials from some of these cases and reported that he had confirmed them bacteriologically as plague. The same confirmation followed at the Hygienic Laboratory on specimens which he sent there. There followed one of the most sordid and distressing stories of sanitary annals, lasting over many months, extending to the

* CALIFORNIA AND WESTERN MEDICINE, in the Lure of Medical History department (issues of November, 1938; December, 1938, and January, 1939), presented the story of the San Francisco Bubonic Plague outbreak in a series of articles by George H. Evans, M. D., who was president of the State Medical Association at the time. To supplement Doctor Evans' account, the comments of Medical Director A. M. Stimson of the United States Public Health Service, Washington, D. C., as recently given in Supplement 141 to the United States Public Health Reports, are given place in this February issue.